discovered by myself, in such widely separated localities as Madeira, Northern Spain, Macedonia, Hercegovina, and the Transcaucasus.

To sum up, the correct nomenclature of our three British species is as follows: —

1. *Platycleis occidentalis occidentalis*, Znr.
1a. *Platycleis occidentalis jerseyana*, Znr.
2. *Metrioptera brachyptera brachyptera*, L.
2a. *Metrioptera brachyptera*, f. *marginata*, Thunb.
3. *Roeseliana roeselii*, Hagenb.
3a. *Roeseliana roeselii*, f. *diluta*, Charp.

SUBSTITUTE FOODPLANTS.

By P. B. M. Allan, M.A.

I have read Mr E. P. Wiltshire's article in the September issue of this Journal, and as it is concerned with a different matter from that which I initiated under this heading in the May 1942 issue, and continued at page 1 of volume lv, I should probably not have penned the following lines had not Mr Wiltshire attributed to me a statement which I did not make and an opinion which I did not express. I remarked, at page 3 of that volume, that *in a particular case* Mr Wiltshire's theory of "common associated evolution of plant and insect" would not seem to apply. Mr Wiltshire has construed (at page 84) this statement as "disproving" the whole of his theory. This is very generous of him, but at present I prefer to reserve my judgment upon his remarkable thesis until he has brought forward the body of evidence which I have no doubt he will presently adduce. It is perhaps a little unfortunate that he should—doubtless inadvertently—have adopted my title for his paper.

That the Lepidoptera evolved "side by side" with *flowering* plants (as I have stated elsewhere) is a postulate which accords with the evidence at present at our disposal; to assert that "plant and insect" have had "a common associated evolution" is quite another matter, and one wonders whether Mr Wiltshire has fully grasped the implications of his new philosophy. Plants form the basis of all life on this earth, because only plants are capable of converting inorganic matter into living matter. *All* animals therefore depend upon plants for their existence, either directly or indirectly. So if one class of animals, to wit insects, has had a "common associated evolution" with plants, Mr Wiltshire will have to inform us why other classes of animals have not enjoyed the same partnership. The advent of mammals is usually associated with the appearance of grasses; does Mr Wiltshire claim that lions and tigers have had a "common associated evolution" with grasses?

With regard to substitute foodplants as a phenomenon in host-selection and biological races, plainly the scientific aspect of this problem is one with which Mr Wiltshire is but little acquainted, since he claims as a coinage a word which is to be found in most of the modern textbooks dealing with the subject, as well as in the scientific periodicals devoted to the biological sciences, both English, American and of other

countries. No doubt he has been handicapped by being obliged to base his premises partly, as he informs us, on a popular handbook for young collectors, published in 1907 and since reprinted many times from stereotype plates.

The literature of oligophagy and polyphagy as phenomena in host-selection and biological races is now a considerable one. Schroeder's observations, published so long ago as 1903, are still very much to the point, and Pictet's classical experiments with the larva of *Lasiocampa quercus*, L., were printed in 1911. Hering gave attention to this subject in his important work on the biology of the Lepidoptera in 1926, and the observations of Marchal (1908), Field (1910), Göschen (1913), Larson (1927), Harrison (1927), Thompson and Parker (1928), and Sladden (1934) should also be consulted. Craighead (1922) and Thorpe (1929-30-31) I have already quoted in a previous article. In 1936 Tate and Vincent discussed the literature of the subject, already large by that time, and an admirable brief résumé of the work done to date was made by Imms in 1937. The names of all the observers in several countries who have devoted their attentions to this subject are too numerous to mention here; their published papers should be studied carefully by anyone who enters upon this field.

The literature of insect palaeontology is also voluminous, so that one hardly knows how to deal succinctly with all the questions which Mr Wiltshire asks on page 84; but, baldly and briefly, the following facts may be of some assistance to him.

The earliest fossil Coleoptera known at present have been found in rocks of the Upper Permian. They consist of two families, the *Permophilidae* and the *Permosynidae*. Of these the first-mentioned appear (*teste* Imms) to be " the direct ancestors of the existing *Hydrophilidae*, while the *Permosynidae* evidently lead on to genera existing in the Upper Triassic rocks." It was a discovery in rocks of this age at Belmont, N.S.W., that enabled the late R. J. Tillyard to describe, in 1924, the tegmen-like elytron for which he erected the order PROTOCOLEOPTERA. Remains of what appear to be even more generalized Coleopterid types occur in the same strata as these ancient beetles. In rocks of the Lower Permian occur the curious beetle-like Protelytroptera. Recent work on the Lower Permian beds of Kansas has been described (1933-5) by Dr F. M. Carpenter. As for the ages of the Permian and Trias, Mr C. E. P. Brooks assesses the age of their bases by duration ratio at 269 and 249 million years respectively. True Coleoptera are abundant in the Trias and include already specialized types.

Mr Wiltshire's question (at page 84), " What palaeontological evidence is there of this genus (i.e. *Cionus*) preceding the Tertiary Age by some two hundred millions years " would seem to indicate some misapprehension. Coleopterid types have been found in the Trias which certain observers have gone so far as to refer to existing Families; but I know of no evidence pointing to the existence of any modern genus in rocks of that age. In his recent (1943) paper, " Studies in the Systematics of Troides, Hubner," Zeuner writes (p. 174): " One will be fairly close to the mark, therefore, if one accepts a period of 500,000 to one million years as the time required for the evolution of a ' good ' species . . . One important point, however, must not be overlooked. If half to one million years are required for the characters of a species

to become stabilized and irreversible, the species, once it has been so formed, may continue to survive virtually unaltered for a very long time thereafter. That this is the case is suggested, for instance, by certain *Hydrophilidae* . . . which have persisted with no apparent change in specific characters since the upper Miocene, a period of about 18 million years." But then, Hydrophilid beetles would, apparently, live in unchanged conditions, as regards environment, for untold periods; so this particular case for unchanged conditions during a period of 18 million years is altogether exceptional. *Prima facie*, such would not seem to apply to a highly specialized group such as *Cionus*, dependent upon terrestrial plants. Presumably even Common Associated Evolution has not stood still so far as the Coleoptera are concerned. That any genus of animals should have existed *in esse* for the tremendous period of 200 million years would be altogether opposed to our present conception of evolution.

But, as I have said, a large amount of work in several countries has been, and is being, done on both these subjects, and I fear Mr Wiltshire will find that much diligent reading will be required to enable him to catch up with it. Perhaps I may add that to propound a new theory of evolution without considerable acquaintance with modern scientific work on palaeontology and biological races seems to be a little precipitate.

COLLECTING NOTES.

An Unusual Pierid Pairing.—On 18.iv.43, in my garden at Rodborough, I netted a pair of Pierids flying *in cop.*, the male being *P. rapae* and the female *P. napi*.—T. Bainbrigge Fletcher, Rodborough, 24.x.43.

Some Dates for Grasshoppers.—The warm Spring of 1943 brought on Acrididae and I give dates when our local species were first noted this year at Rodborough (600 feet, on oölite), with some early and late dates of previous years:—*Stenobothrus lineatus*, ♂♀, 13.vi (14.vi.38, 27.x.37); *Omocestus viridulus*, ♂ 11.vi, ♀ 13.vi (11.vi.40, 9.x.37); *O. ventralis*, not seen in 1943, not at all common here (12.viii.39, 4.x.37); *Myrmeleotettix maculatus*, 28.v (13.vi.38, 2.xi.37); *Chortippus bicolor*, ♀ 28.vi (28.vi.40, 2.xi.37); *C. parallelus*, ♂ 12.vii (♂ 6.vii.38, ♀ 18.x.37); *Gomphocerus rufus*, ♂♀ 28.vii (30.vii.40, 11.xi.37, and one, still immature, on 26.xi.37).—T. Bainbrigge Fletcher, Rodborough, 24.x.43.

Non-Specific Assembling Scents in Macro-Lepidoptera.—With reference to Dr Kettlewell's interesting notes in the *Ent Rec.*, lv, 107, and liv, 62, it may be worth while to record that in 1940 Mr J. C. F. Fryer, who was breeding *Spilosoma lutea*, Hufn., from Wood Walton Fen, noticed that there were as many as twenty *Arctia caja* males in the insectary one morning, although there were no females of the latter in any of the cages, and they continued to appear as long as the *S. lutea* were emerging. The insectary is a wooden building about 18 feet by 9 feet 6 inches with wire netting sides standing in the laboratory garden. The same thing happened in 1941 and 1942. In case there should have been any wild females of *A. caja* anywhere near the insectary, the cage containing the *S. lutea* was in 1942 moved to an open greenhouse in